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THE EFFECTS OF PREDETERMINED PAYMENT
RATES FOR HOME HEALTH CARE

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ABSTRACT

By paying agencies a prospectively-set rate for home **health** visits, the Home Health Per-Visit Prospective Payment Demonstration introduced an incentive for agencies to control their costs of **delivering** Medicare home **health** visits. Our study of the 47 agencies that participated in the demonstration shows that this new incentive was largely overwhelmed by the current home health environment, which is characterized by diversity, change, and competitive pressures. Nonetheless, the opportunity to earn a profit (and the increased possibility of losses) may have increased slightly the level of attention agencies gave to cutting costs. The agencies randomly assigned to prospective rate setting were more likely than the control group to hold their cost increases below inflation, enabling three-fourths of the treatment group to earn profits, but the average size of the real cost reduction was small (about 4 percent). These effects were limited to agencies that served predominantly Medicare patients and were not controlled by a hospital or hospice. Prospective rate setting had no discernable effect on the number of visits provided by agencies, or on patients' other Medicare costs, quality of care, access to care, or use of services not covered by Medicare.

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THE EFFECTS OF PREDETERMINED **PAYMENT RATES FOR HOME HEALTH CARE**

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Rapid growth in home health care costs figures prominently in the current national debate about ways to slow growth in total Medicare costs, but home health care costs have been increasing at an alarming pace since 1988. This study evaluated a demonstration of an alternative payment method designed to encourage home health care agencies to lower their average cost per visit. We examined the demonstration's effects on agencies' cost per visit, volume of services, quality of care provided, and selection of patients. We also looked at agencies' profitability under the demonstration and effects on patients' use of various medical and social services.

GROWTH IN HOME HEALTH COSTS

Between 1983 and 1994, total Medicare outlays for home health care increased **from** \$1.6 billion to \$13 billion, raising home **health's** share of **total** Medicare costs from 2.8 percent to 7.8 percent. Costs per beneficiary increased from \$78 to \$352 in 1993 dollars during this period. This rapid increase warrants exploration of alternatives to the current cost-based method of reimbursing home health agencies.

The **major** source of the increase was in the average number of visits per episode, which more than doubled during this period, from 28 to 65. All of this increase occurred between 1989 and 1994 (see Table 1). The proportion of beneficiaries who use home **health** also nearly doubled between 1983 and 1994, from 4.5 percent to 8.7 percent. Again, the increase was concentrated in the last five years. The explosion in **visits** per episode is almost surely attributable to the 1989 revision of the Medicare regulations arising **from** the *Duggan v. Bowen* decision, which reinterpreted the coverage guidelines to allow skilled nursing and accompanying aide care for chronic conditions, rather than only for acute conditions. The decision also relaxed the restrictions on providing multiple visits per day.

TABLE 1
MEDICARE HOME HEALTH USE, 1983-1994

Year	Total Medicare Home Health Expenditures (Parts A & B in Billions)	Total Number of Medicare Beneficiaries (in Millions)	Home Health Users Per 1,000 Beneficiaries	Visits Per Home Health User	Payment Per Visit
1983	61.6	29.9	45	28	\$43
1984	\$1.9	30.4	\$0	27	\$46
1985	61.9	31.0	50	26	\$49
1986	\$1.9	31.6	50	24	\$51
1987	\$1.9	32.2	48	23	\$51
1988	\$2.1	32.9	48	24	\$56
1989	\$2.6	33.5	50	27	\$56
1990	\$3.9	34.1	57	36	\$57
1991	65.7	34.8	64	45	\$57
1992	\$7.9	35.5	71	53	\$59
1993	\$10.7	36.2'	80	60'	\$61
1994	913.0	36.9	87	65^u	\$62^a

SOURCE: Prospective Payment Assessment Commission (ProPAC), June 1995. Figures are not adjusted for inflation.

^aEstimated.

^uRecent unpublished HCFA calculations yield somewhat smaller estimates of visits per home health user, 57 for 1993 and 58 for 1994.

\$ 190 per home health user not paid

In contrast to these large increases in usage rates and visits per episode, the Medicare payment per visit grew by only 44 percent since 1983. This rate is **virtually** identical to the increase in the overall Consumer **Price** Index (**CPI**) during this period and less than half the 100 percent increase in the medical care component of the CPI. The relatively slow growth in cost per visit is due partially to the increasing proportion of home health visits rendered by aides, which are only half as expensive as nursing or therapy visits, on average. Per visit costs for specific types of visits, however, have also grown slowly. The average cost for a skilled nursing visit grew only about 3.2 percent per year between 1987 and 1993 for our control group; the average cost for an aide visit grew by less than 1 percent. The Health Care Financing Administration's (**HCFA's**) data on agency costs and cost limits show similar rates of increase in median cost per visit for all agencies nationally between 1986 and 1989 (a 4.6 percent annual increase for nursing, and a 1.6 percent decrease for aide visits). The large increases in agencies' size between 1989 and 1993 enabled many to lower their per visit cost, while the Medicare cost limits have constrained the amount paid by HCFA for many others. These cost limits cap the amount that HCFA pays to an agency at 112 percent of the national mean cost for the mix of visits provided.'

Although controlling home health costs today will probably require a payment methodology that encourages fewer visits, controlling growth in the average cost of a visit will be important under any payment method. For example, the Medicare reform bill passed by the U.S. House of Representatives calls for paying agencies a fixed price per visit, putting a ceiling on the average cost per episode for care delivered by agencies during the course of a year and sharing savings with agencies that hold costs below the ceiling. Agencies' ability to control cost per visit will influence whether they survive under a tighter ceiling and the amount of the saving to be shared with HCFA

'The cost limit for an agency is equal to the sum over the 6 types of visits of the number of visits of a given type times 112 percent of the national average cost for that visit type.'

The wide variation across agencies in cost per visit suggests that some agencies are not delivering

what is the product being delivered?

I-- home health care efficiently under cost-based reimbursement. For skilled nursing visits, 10 percent of agencies had an average cost per visit below \$46 in 1990, while another 10 percent had costs in excess of \$105 per visit (see Table 2). The cost of a home **health** aide visit varied even more, with those at the tenth percentile having a per visit cost only about one-third that of the agencies at the 90th percentile. Although some of the disparity in per visit costs is the result of differences in local labor costs, total costs in approximately one-third of agencies exceeded the Medicare cost limits, which are adjusted for area local **differences** in labor costs. In another 39 percent, total costs were more than 15 percent below the limits. Agencies above the limits presumably already have an incentive to deliver care more efficiently, but in some cases, these agencies have other organizational incentives that dominate breaking even on their home health businesses. These agencies may seek to maximize gross revenue from Medicare, which would mean having reported costs that equal or exceed the cost limits. There is no incentive under cost reimbursement for agencies to have costs substantially below the cost limits.

non-Medicare patients

out of pocket payments

THE DEMONSTRATION

The per-visit prospective rate setting demonstration, originally scheduled to begin in 1985, did not actually begin until fall 1990. The delay was due to the Office of Management and Budget concerns that the proposed demonstration, which included a test of both prospective payment per episode and prospective rate setting for home health visits, contained an inadequate case mix adjuster for the **per-episode** component. HCFA then decided to test the two payment types in separate demonstration phases. The first examined the effect of paying agencies a predetermined per-visit rate for each type of home health visit. The second phase, which tests prospective payment for episodes of home care, began in mid-1995.

Under the per-visit demonstration, agencies are paid a prospectively determined rate for six types of home health visits, with adjustments for sizable changes in volume. Profits and losses are shared with HCFA. This payment methodology varies substantially from the current payment method, which pays

TABLE 2

DISTRIBUTION OF COST PER **VISIT** FOR DIFFERENT **VISIT** TYPES, 1990
(In Dollars)

	10th Percentile	90th Percentile
Skilled Nursing	47	105
Home Health Aides	19	54
Physical Therapy	46	114
Occupational Therapy	45	120
Speech Therapy	45	117
Medical/Social Services	49	232

SOURCE: HCFA Cost Limit Files, Cycle 11.

agencies as services are rendered and reconciles payments to actual costs when agencies submit their annual cost reports. HCFA set the prospective rate for an agency participating in the demonstration at the agency's cost per visit in the base year (the agency's fiscal year preceding entry into the demonstration), adjusted **for** expected inflation. If the agency's total base-year costs exceeded the cost limit for that year, the base-year rates for each type of visit were reduced by the ratio of the agency's base-year cap to its actual base-year costs.

Payments were adjusted at year-end for demonstration agencies that experienced (1) sizable changes in the number of Medicare visits rendered relative to their base year, or (2) large profits or losses. Per-visit reimbursement rates were reduced one percent for agencies whose total number of Medicare visits increased by 10 to 20 percent. Rates were increased by one percent for agencies whose volume declined by 10 to 20 percent. Each additional 10 percentage point change in volume added an additional 1 percent to the change in the reimbursement rate, up to a maximum 5 percent change for agencies whose volume increased or decreased by more than 50. percent. These adjustments for volume were intended to reflect the effects of economies of scale on agencies' average costs, and to discourage agencies from increasing their volume. The profit- and loss-sharing provision required agencies to return some part of their profits on Medicare visits, if profits exceeded five percent of Medicare-allowable costs, according to the following schedule for the first demonstration year:

Profit as Percentage of Allowable Costs	Percentage Returned to HCFA
5% to 15%	25 percent of profits in this range
15% to 20%	50 percent of profits in this range
More than 20%	100 percent of profits in this range

The 20 percent ceiling on profits was raised to 25 percent in year 2 of the demonstration and to 30 percent in year 3. HCFA reimbursed agencies for losses in excess of five percent of cost, up to the point at which

total payments to the agency equaled the cost limit. These provisions reduced the financial risk to participating agencies and allowed HCFA to share in net savings if they were sizable.

The opportunity to earn a profit was expected to motivate demonstration agencies to hold their increase in cost per visit below the inflation rate used to calculate their payment per visit, but raised concerns about potential adverse effects on costs, patients, and caregivers. Agencies were expected to make a variety of changes to enhance efficiency and hold down both service-related and **administrative** costs. Costs to the Medicare program may actually increase under prospective rate setting, however, if agencies respond to the profit potential by providing more total visits than they would have delivered under cost reimbursement, or if agencies' efforts to lower per-visit costs also lowers the quality of care and results in higher use of other Medicare-covered services (such as hospital, skilled nursing facility, physician **services**, or emergency room care). Patients could also be adversely affected either medically or financially by agencies' responses to the incentives. Lower quality of care resulting **from** shorter visits, less supervision, or use of less-qualified **staff** could lead to poorer functioning and other health outcomes for patients. Alternatively, patients might need to obtain more formal (paid) or informal (unpaid) care if **Medicare** home health care becomes less effective or meets fewer needs, which would increase patients' out-of-pocket cost and/or caregivers' burden. If agencies that are 'paid a prospectively set rate begin to **avoid** patients requiring longer, more expensive visits, some patients' access to care might also decline.

The demonstration, which was open to all nongovernment home health agencies that had been in **operation** for at least three years in five states (**California**, Florida, Illinois, Massachusetts, and Texas), attracted only 47 agencies, far less than the 67 that HCFA sought. In each state, the participating urban agencies **within** each of three strata (freestanding **proprietary**, freestanding voluntary or nonprofit, and facility-based agencies) were randomly assigned to treatment or control status as they entered the

demonstration.² Treatment agencies were paid according to the demonstration rules described earlier; control agencies were paid under the usual cost-based method. Twenty-six of the participating agencies were assigned to the treatment group, 21 to the control group (the unequal numbers were due to an odd number of agencies in some strata at the time of randomization). An agency participated in the demonstration **for** three years, with participation beginning at the start of the agency's next fiscal year after application. The first agencies began demonstration operations in October 1990; the last began in October 1991.

The participating agencies differed somewhat from a random sample of agencies nationally that met the demonstration eligibility criteria (except for state), and **from** agencies in the demonstration's states that were eligible but chose not to participate. The proportion of demonstration agencies that were for-profit was significantly higher, and the proportion of hospital-based agencies was significantly and markedly lower than the corresponding proportions of agencies nationally (see Table 3). Demonstration agencies were smaller on average, delivering about 11 percent fewer visits than agencies nationally in 1990. Demonstration agencies' average costs per visit were very similar to the national averages, however, for each visit type. Participating and nonparticipating agencies also had similar means and distributions of the ratio of actual costs to cost limits for 1990. Comparison of demonstration agencies to eligible agencies within the five demonstration states (not shown) yielded essentially similar findings.

The **patient** mix for demonstration agencies was remarkably similar to that for the national sample of agencies, but they operated in different environments. Patients had similar demographic characteristics, prior use patterns, and diagnoses at admission (see Appendix Table A. 1). Area characteristics, such as **urbanicity**, population, and hospital wage index, differed considerably, however (Appendix Table A.2). The differences are due almost entirely to the proportion of rural agencies. About one-third of agencies nationally are rural, compared to only 13 percent of our sample. When the demonstration agencies are

²The six rural agencies in Illinois and Texas that applied formed a separate stratum.

TABLE 3

**CHARACTERISTICS OF PARTICIPATING AND ELIGIBLE
NONPARTICIPATING AGENCIES, 1990**

Variable	Participating Agencies	Eligible Nonparticipating Agencies	Difference	p- Value
Number of Agencies	47	1992		
Proprietary Status (Percentage)				
For profit	57.5	36.0	21.4	.00**
Provider Type				
Hospital-based	12.8	38.4	-25.6	.00**
Visiting nurse association	17.0	14.5	2.5	.63
Other freestanding	70.2	47.1	23.1	.00**
Number of Visits--Medicare and Non-Medicare				
Total	19,421	21,863	-2,442	.70
Nursing	10,060	9,784	276	.92
Home health aide	6,978	9,583	-2,604	.42
Cost per Visit--Medicare and Non-Medicare (dollars)				
Total	58	58	-.48	.91
Nursing	75	74	1.54	.79
Home health aide	36	35	.85	.84
Average Number of Visits Per Episode--Medicare	27.8	26.8	1.0	.65
Ratio of Cost to Cost Limit				
Mean	0.99	0.94	0.04	.42
Distribution (Percentage)				
Under 85 percent	45.2	39.3	5.9	.51
85 to 100 percent	22.6	28.5	-6.0	.47
100 to 115 percent	19.4	17.2	2.1	.76
Over 115 percent	12.9	14.9	-2.0	.75

SOURCE: HCFA Cost Limit Files, Cycle 11 (for fiscal years ending between July 1989 and July 1990), Provider of Services file, and Medicare claims data for 1990.

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

compared to nonparticipating agencies in the demonstration states, excluding all rural agencies except those from Texas and Illinois (the two states with rural agencies participating in the demonstration), the differences disappear.

The differences between the participating and nonparticipating eligibles suggest that our estimates may not be readily generalizable to the population of all home health agencies nationally. The cost and volume differences are not large enough to create concerns about the usefulness of the results for predicting effects of prospective rate setting on other agencies. If effects differ between facility-based and freestanding agencies, however, our estimates give too little weight to the facility-based agencies to draw accurate inferences about a national program. There are too few observations on facility-based agencies to estimate impacts for this group separately. Similarly, rural agencies are underrepresented. Thus, the results are mainly indicative of the effects of prospective rate setting on urban, **freestanding** agencies.

EVALUATION METHODOLOGY AND DATA

To assess whether and how potential demonstration effects occurred and estimate their size, we conducted case studies of agency behavior and analyzed primary and secondary data from several sources. The combination of data sources and coupling of qualitative and quantitative analyses ameliorated the weakness of having so few agencies.

Case Studies and Site Visits

To **learn firsthand** how agencies responded to the demonstration incentives, we conducted two detailed case studies, based on interviews with 22 agencies selected judgmentally to represent all demonstration **participants**. Agency **staff were** interviewed twice, once between July and October 1991 (during the first year of operations), and once between June and April of 1993 (the third year of the demonstration). Staff at treatment group agencies were interviewed in person; staff at control agencies were interviewed by telephone, using a shortened version of the interview protocol to guide the discussions. We spoke with

chief executive officers, chief financial officers, clinical supervisors, nurses, and therapists about a broad set of issues, including agency structure, financial performance, local market issues, cost-cutting behavior, and quality assurance.

The case studies also drew on our annual mail survey of demonstration agencies and interviews with staff at HCFA, Abt Associates (the implementation contractor for the demonstration), and the fiscal intermediary for the demonstration. The agency survey collected information on personnel, patient and area characteristics, staff supervision and training, referrals, and revenue sources. Interviews with other actors provided information on program operations, agencies' reasons for participation, and other useful background on agencies

Data Sources for Impact Analyses

The impact evaluation, which was based on statistical comparisons of the treatment and control agencies and their patients, required data on outcomes at both the agency and patient level. At the agency level, agencies' annual cost reports were the key source of data on outcomes. These reports provided information on the number and average cost of visits, by type of visit, plus agency revenues from Medicare and non-Medicare sources. For each agency, these data were collected for the three demonstration years and the three years preceding the demonstration. Information on patient outcomes was drawn from a variety of sources, including demonstration claims and Medicare claims files for all patients in participating agencies, as well as patient surveys conducted for a sample of patients at discharge and six months after discharge. The claims data, which supplied information on patients' use of services and costs, were used to construct episodes of care (the unit of analysis for estimates based on individual-level data), and services used during and after those episodes. The discharge and followup surveys provided information on patient satisfaction with care, functioning, other measures of patients' well-being, and use of non-Medicare services, both during and after the episode. A separate survey conducted at three weeks after admission

provided information on visit length. Data collected by the New England Research Institute (**NERI**) to monitor the quality of care provided measures of quality for our analyses as well.

The impact analyses also required data on control variables, which were used in our statistical models to account for exogenous differences between the treatment and control groups of agencies or their patients. Area characteristics (such as number of physicians per 1,000 area residents and nursing home beds per 1,000 elderly people) were obtained **from** the Area Resource file. Agency characteristics, such as for-profit status, auspice (whether private free-standing, visiting nurses association, or hospital-based), and location were obtained from the demonstration contractor. The patient-level analyses also controlled for patient characteristics at admission that might affect outcomes. Control variables for patients admitted during the demonstration period were drawn **from** four sources: (1) a patient intake form, developed for this demonstration, that provided information on patients' functioning, diagnosis, care needs, referral source, and prognosis at the time of admission to home **health**; (2) **HCFA's** plan of treatment forms (**485s**), **which** provided data on treatments planned at the time of admission; (3) prior Medicare claims, which provided data on patients' use of Medicare-covered **services** in the 12 months preceding admission; and (4) Medicare's master beneficiary file, which contained basic demographic characteristics (age, sex, and race). Each episode of home health care was treated as an independent observation. Episodes were assumed to begin with the start of care date on the **claims** and to end with the "services through" date on the last claim with that start of care date.

Because the patient-level data were available for essentially all episodes of care that demonstration agencies delivered during the three-year period, sample sizes were very large for the claims-based analyses (Table 4). Over 88,000 home health episodes were included in the analysis. These episodes account for approximately 80 percent of the total number of admissions to these agencies during the demonstration period. The loss of observations was due to difficulty in matching data from demonstration claims to data **from** plan of treatment and patient intake forms, which were often missing, and to the exclusion of patients

admitted to an agency during the last three months of its third demonstration year or **after** January 1, 1993.

(Because of billing and processing lags, the data for these episodes were incomplete.)

A sample of patients was **surveyed** at discharge and again six month later to obtain data not available from secondary sources. The sample for the discharge survey was drawn **from** patients discharged between January 9 and March 26, 1993, the beginning of the third demonstration year for most agencies. Ninety-one percent of the selected sample completed the interview, yielding 2,059 observations. The sample sizes used in the analysis were smaller, however, because of the inability to link some observations to the patient intake forms, **485s**, and claims data required for some of the control variables. Table 4 contains the sample sizes used in analyses of the patient discharge survey and the follow-up survey. Ninety-four percent of those completing the discharge survey completed the **followup** survey.³

Statistical Methodology

The **different** units of analyses and types of data available required a variety of statistical procedures. The basic principle, however, was the same for all analyses: to compare the experience of treatment and control groups, using statistical models to account for any observable, exogenous differences between the two groups that might have remained despite random assignment.

The agency-level analysis of costs per visit and number of visits used fixed-effects models to estimate program effects on the change between the three demonstration and three predemonstration years. The impact estimate is equivalent to the average treatment-control difference during the demonstration period minus the average **difference** during **the** predemonstration period. No control variables other than time and agency binary variables were included in the basic analysis. Constant agency characteristics that would be obvious variables to control for (such as for-profit status) drop out of these models, and time-varying

³No attempt was made to conduct follow-up interviews with proxies for sample members who were deceased at the time the discharge survey was fielded.

agency characteristics were **potentially endogenous** or so crudely measured that they would contribute little or nothing to the explanatory power of the model.

Impacts on net revenues were estimated using regression models, controlling for agency and area characteristics. We estimated models for each demonstration year separately and for all years combined.

The patient-level analyses were based on regression models estimated separately for each demonstration year and for the entire demonstration period. **Logit** models were estimated for binary dependent variables; **tobit** models were estimated for variables truncated at zero. In each of these models, observations were weighted so that each agency was represented equally, because the intervention was at the agency level. We also estimated models in which agencies were represented in proportion to the number of episodes they delivered

Throughout the analysis, hypotheses about program effects were tested using two-tailed tests at the five percent significance level (except for tests of effects on cost per visit, which were one-tailed). To avoid overstating the precision and statistical significance of our finding, standard errors of the estimates obtained from the patient-level data were adjusted for sample design effects of **clustering**⁴ and weighting. We used **SUDAAN**, a specialized program designed for such purposes, to calculate the design effect for some key outcome measures and then **inflated** the variances used for all of our statistical tests by the average design effect for the related key variables. Design effects for the claims sample were typically about 9, indicating that standard errors were underestimated by a factor of 3. Thus, impact estimates based on Individual-level data were not statistically significant at the five percent (two-tailed) level unless the uncorrected t-statistics reported by the statistical software exceeded 5.88 ($3 * 1.96$). For the survey sample,

⁴**Because** all of our 88,000 observations on home health episodes are drawn from only 47 agencies, the observations are not independent. Accounting for the effect of this interdependence increases the variances of estimates relative to those from a simple random sample by a sizable factor (the “design effect”) when the number of observations per agency is large.

uncorrected standard errors were underestimated by 40 percent for analyses of functioning and by 20 percent for the analyses of satisfaction and use of formal and informal care not covered by Medicare.

Methodological Problems and Weaknesses

Despite the strength of a true experimental design the study **suffered** from several problems, the most critical being the small number of agencies. With only 47 agencies, the analyses based on agency-level data had little statistical power. The precision of the estimates was enhanced by the use of data for six years, but the number of agencies was still too small to detect anything but fairly large effects on agencies' cost per visit or volume. For example, we estimated that we had an 80 percent or greater probability of detecting impacts on per-visit costs for skilled nursing only if the true effects exceeded 11 percent. The small sample provided almost no ability to assess whether effects varied with key agency characteristics (such as for-profit status or size).

A second problem was the skewed distribution of agency size. Agencies in the demonstration delivered anywhere **from** 120 to 330,000 visits per year. This disparity led to numerous problems in the analysis of patient-level data **If** observations were not weighted, the results would be dominated by the experience of the few very large agencies (indeed, one agency accounted for 40 percent of the episodes of care delivered by the 26 treatment group agencies). Weighting patient observations so that each agency was represented equally sometimes led to anomalous estimates, however, The patients served by a small agency in a particular year might not have been very representative of either small agencies in general or that same agency in other years, but the agency's mean implicitly received the same weight as the mean for an agency with 100 times as many episodes. This problem was especially severe for the survey sample analyses because we had very few observations on several agencies. Even the agency-level analyses were affected by problematic data for small agencies; average per-visit costs and number of visits for small agencies often fluctuated considerably from year to year.

A third concern was losing a sizable number of observations in the individual-level analyses, which resulted **from** inability to link data from the plan of treatment forms, patient intake forms, and Medicare claims **files** to the demonstration claims **files** and the survey files. Patient identification numbers and names on the intake forms were often recorded incorrectly, and many agencies did not submit all patient intake forms and 485s. The sample sizes were still large for the analyses of outcome measures gathered **from** claims, but the loss in sample size for analyses of survey measures reduced precision levels noticeably.

Finally, observations on a substantial number of patient episodes were truncated, either because of the end of the demonstration or the need to cut off data collection in order to complete the analysis. A significant number of patients were in the midst of an episode of home care at the time the demonstration period ended for their agency. The end of the demonstration meant that these episodes would be completed under cost-reimbursement rules. Thus, there would not be a clear interpretation of the **findings** if those episodes were included. Furthermore, the detailed data on types of home health visits provided were only available for the demonstration period. Other episodes were in progress when we extracted data **from** the claims files to construct the final analyses files. Simply deleting the truncated observations was not appropriate, because these patients tended to have long episodes. Treating the truncation point as the **end** of the **episode** was equally inappropriate. Our approach excluded **from** the claims file all episodes that **began after** December 1993, plus all other episodes that began in the last three months of an agency's **final** (third) year of demonstration participation. This approach greatly reduced the number of episodes that were truncated at the end of data collection or the end of the demonstration. We modified our statistical models to account for **remaining** truncation effects (see Schochet 1995 for a thorough description of these models).

EFFECTS ON AGENCY BEHAVIOR AND POTENTIAL COST SAVINGS

The evidence from our impact analyses and **interviews** with agency **staff suggests** that a number of home health agencies in the treatment group made a few modest changes in behavior in response to the

demonstration's financial incentives. The limited changes enabled many agencies to earn a small profit by holding increases in their cost per visit, relative to base-year costs, below the HCFA-specified inflation factor (about five percent per year). We found no **consistent** evidence that agencies paid under prospective rate setting had noticeably lower costs per visit, however, than they would have had otherwise. We also found that the demonstration had no effect on the number of visits that agencies provided or on patients' need for other Medicare-covered services. Thus, total costs to Medicare were **unaffected** by prospective rate setting.

Agencies Made Few Changes

In our agency case studies, we found that both treatment and control agencies expressed concern about efficiency and about financial survival, but that most identified little that could be done to reduce their costs. The first case study, conducted during the first demonstration year, found agencies adapting to the demonstration, but worrying more about competition for patients and staff than reducing their costs (see Thornton et al. 1992). Our second case study reinforced the perception that few changes had been made to constrain cost increases (Thornton et al. 1993). Agencies said many factors that influence the provision of care were unchanged, despite the change in payment. Medicare rules and state requirements; demands by hospitals, physicians, and other referral sources for extensive, high quality home health care; professional standards for nursing staff, and competition for patients and staff made it difficult for agencies to cut back on clinical costs.

Agencies in the treatment group liked prospective rate setting because of greater certainty in the payment levels it provided, but their behavior differed only slightly from that of control agencies. The treatment group made greater purchases of and use of computers to improve efficiency, and they also expected salaried nurses to make more visits per day, on average (5.3 versus 4.8). They were also somewhat less likely to raise hiring standards or to increase training. The differences were relatively

TABLE 4
SAMPLE SIZES FOR CLAIMS AND SURVEY DATA

	National Claims History Data			Survey Data	
	Year 1	Year2	Year 3'	Discharge	Followup
Treatments	24,043	23,484	15,881	789	656
Controls	9,565	10,175	5,552	801	695
Total	33,608	33,659	21,433	1,590	1,351

NOTE: Actual sample sizes vary because of missing data for some dependent variables.

'Fewer Year 3 observations are available because 8,603 episodes that began either during the last three months of Year 3 or after December 31, 1993, were excluded from most analyses to minimize the number of observations for which data were truncated by cutoffs in data collection.

modest, however, so the shift to prospectively set rates was not likely to generate marked decreases in costs.

Evidence Suggests No Cost Savings

To assess **statistically** whether the modest changes in behavior led to discernible effects on costs, we estimated fixed-effects models of the average cost per visit, for each type of visit. The model estimated impacts as the treatment-control **difference** in the change in average cost per visit between the three-year demonstration and predemonstration periods:

$$(1) Y = \sum a_i t_i + \sum b_j A_j + \sum c_i (t_i * T) + e,$$

where the t_i are **binary** variables indicating the year of the observation $i = 1, \dots, 6$, with the binary for the base year, $i = 3$, excluded), the A_j 's are binary variables indicating agency, T is a binary variable equal to 1 for agencies in the treatment group, and e is a random error term. Impacts were estimated as $(c_4 + c_5 + c_6 - c_3)/3$, reflecting the three-year demonstration and predemonstration periods. The coefficients on the **binary** time variables (a_i) captured secular trends in the outcome measures; the b_i 's captured the effects of any constant agency-specific differences in underlying practice patterns, patient characteristics, and market factors. Bishop et al. (1995a) provide full details on the estimation strategy and results.

The estimated effects were not significantly different from zero for any visit type. The results, displayed in Table 5, indicate that cost per skilled nursing visit for treatment agencies increased from an average of about \$81 to about \$92 between the predemonstration and demonstration periods, or about 17 percent (an average annual increase of 5.4 percent between the two periods). Control agencies' average costs were somewhat lower than treatment agencies' prior to the demonstration but grew by a similar amount. The estimated impact, \$1.67, was positive, small, and not significantly different from zero. Similarly, estimated effects on the cost of home health aide visits and therapy visits were small and not

TABLE 5
COST-PER-VISIT INCREASES AND IMPACTS
(In Dollars)

	Predemonstration Mean		Estimated Increase		Predicted Mean without Intervention	Impact	t-Statistic
	Treatment	Control	Treatment	Control			
Skilled Nursing	80.60	70.17	11.71	10.05	85.69	1.67	.43
Home Health Aide	37.44	36.06	1.41	.63	42.01	.78	.28
Physical Therapy	79.83	70.53	12.21	8.07	78.76	4.14	.95
Occupational Therapy	74.67	83.73	13.67	11.33	86.51	2.34	.24
Speech Therapy	83.61	74.28	9.02	4.60	81.32	4.41	.77
Medical Social Services	149.47	99.65	-9.79	10.09	133.40	-19.88	-.84

SOURCE: Bishop et al. (1995a), based on data from 47 agencies' Medicare cost reports for three years before and three years during the demonstration.

NOTE: All estimates were obtained from a fixed-effects regression model. Increases are the estimated differences in means between the demonstration period and predemonstration period, unadjusted for inflation.

significantly different from zero. The estimated effect on the cost per unit of medical social services was **fairly** large and in the expected direction (-\$20, or about 15 percent of the predicted cost in the absence of the intervention). The effect was statistically insignificant, however, reflecting the large variation in the average cost of these visits.

We also tested for whether demonstration effects on **total** cost per visit were being masked by economies of scale or treatment-control differences in patient mix. Treatment agencies were somewhat larger, on average, than control agencies in the base year. Several studies (Hay and Mandes 1984; Kass 1987; **Nyman** and Svedlik 1989; **Schmitz** 1990; and Chu et al. 1993) have found sizable economies of scale in home **health** care. **If these** economies are greater at lower volume (or the treatment agencies grow at a slower rate), impacts on costs could have been masked because volume differences were not taken into account. Similarly, patient mix could have changed differently over time for treatment and control agencies in ways that could have masked demonstration effects. Although the influence of demonstration-induced volume and case mix changes on cost are part of the overall effect of the demonstration, we were also **interested** in whether treatment agencies delivered visits less expensively apart from these changes. Thus, we **re-estimated** the cost models, controlling for agencies' number of visits by type and several measures of average patient characteristics obtained from Medicare claims files, including average age, percentage **admitted** to home health within two weeks after a hospital discharge, percentage with diagnosis associated with long **visits**, percentage with diagnoses linked to high total costs per episode, average number of hospital stays in year preceding home health admission, average length of hospital stay prior to home health, and the proportion admitted to a hospital within 30 days after home health discharge.

The estimates of impacts on average costs that controlled for agency volume and patient mix (not shown) remained small and statistically insignificant, suggesting that average costs were unaffected by the demonstration. Some of the estimates changed signs, but none was remarkable.

Impacts did not appear to exist for subgroups of the treatment agencies that might either have been more able to respond to **demonstration incentives** or more aggressive about pursuing profits. Impacts were estimated for subgroups **defined** by whether the agency was **free** standing versus hospital-based, size (whether more than 10,000 visits), for-profit status, and whether a Visiting Nurse Association agency. For each subgroup examined, we found no statistically **significant** effects for any visit type, with two exceptions: (1) among larger agencies, cost per visit for medical social services was lower for treatment group agencies; and (2) among not-for-profit agencies, cost per visit for skilled nursing was lower for treatment group agencies. These effects both appear to be statistical anomalies, occurring at roughly the expected **frequency**, given that tests were conducted at the five percent level for a one-tailed test (we have 42 estimates, six visit types in each of the seven subgroups examined). We saw no reason to expect that nonproprietary agencies would be better able or more willing than proprietary agencies to cut their costs in order to **earn** a profit under the demonstration; our expectation was that the proprietary agencies would be more aggressive about cost cutting. We also did not believe that large agencies cut their medical social **service** costs substantially in response to the demonstration but were unable to influence the costs of any other type of visits. In both cases, the impact was significantly different **from** zero for the subgroup, but we could not reject the hypothesis that the impacts for the two subgroups were significantly different **from** each other.

The lack of significant effects on cost per visit may have resulted **from** the weak power of the analysis created by the small sample sizes, but the results did show no sizable effects on cost. Our sample has 80 percent power to detect effects on skilled **nursing** costs only if they exceeded \$9.62, or about 11 percent of the predicted mean cost that would have been observed for sample agencies if the demonstration had not occurred. For home health aide visits, only true effects of \$6.93 or larger (16 percent of the mean) were detectable at 80 percent power. Although modest effects on cost of 5 to 10 percent may have occurred but gone undetected because of sample variation, it is highly unlikely that the demonstration

generated cost reductions in excess of 10 percent. The small size of ~~the~~ estimates, together with ~~the~~ findings from our site visits, suggested that any cost reductions were quite small.

No Effects on Agency Volume

One concern about the demonstration was ~~that~~, regardless of the effect on costs per visit, overall costs to HCFA might rise under prospective rate-setting because agencies might increase the number of visits they provided by more than they would have under cost-based reimbursement. Increased growth could have occurred if treatment agencies (1) believed that they were able to hold costs down below the preset payment rate and sought to earn greater profits by increasing volume; (2) hoped to lower costs by reaping greater economies of scale through growth; or (3) exploited the fixed rate per visit by providing in two short visits ~~services~~ that would have been provided in a single, longer visit under cost-based reimbursement. Growth that resulted from demonstration agencies attracting patients away ~~from~~ other ~~agencies would~~ not necessarily have increased overall costs to HCFA, and it would have decreased them ~~if agencies receiving~~ prospectively set rates drew patients away ~~from~~ higher-cost agencies. If the increased visits resulted from agencies increasing the number of visits per episode beyond what they would have ~~provided~~ had compensation been on a traditional cost basis, however, costs to HCFA might have risen.

To assess demonstration effects on total agency volume and visits per episode, we conducted analyses ~~at~~ both the agency and ~~episode~~ level. The agency-level analysis involved estimating impacts on total visits ~~provided~~, by type of visit, using the same data sources and fixed-effects models that were used to assess ~~impacts~~ on cost per ~~visit~~. The dependent variable (number of visits) was expressed in logarithmic form to account for heteroskedasticity and for the likelihood that demonstration impacts, if any, were likely to be proportional to an agency's normal volume. Impacts on visits per episode were estimated using episode-level data, with separate analyses conducted for each demonstration year on the large episode-level samples. We also estimated effects on episode length, frequency of visits (visits per week), and intensity of visits (length of visits). The methods and results are described more fully in Bishop et al. (1995b).

Agencies participating in the demonstration, like most agencies nationally, grew very rapidly during the demonstration period. More than half doubled their number of visits between the base year and the end of the demonstration. On average, agencies increased their volume by over 20 percent per year during the demonstration period (excluding one agency that grew to more than 200 times its base-year size). The rates of increase were similar for the treatment and control groups. The increase was especially large for home health aide visits (25 to 30 percent per year on average).

The evidence suggests that prospective rate setting had no impact on volume. Although treatment group agencies delivered about 50 percent more visits than control agencies, on average, in the predemonstration period, average total visits for the two groups grew at similar rates between the base year and the end of the demonstration—21.3 percent per year for the treatment group and 23.6 percent per year for the control group. Table 6 estimates the impact on average growth rates for total visits and for nursing, aide, and physical therapy visits separately (these three visit types account for about 96 percent of all home health visits). We estimated the effect on the annual rate of growth as the estimated treatment-control difference in growth rates over the demonstration (columns 1 and 2 of Table 6) minus the difference in growth rates over the predemonstration period (column 3 of Table 6). We find that treatment agencies grew slightly more slowly than controls during the demonstration (2.3 percent per year) and slightly more rapidly during the predemonstration period (2.5 percent per year), yielding an estimated effect of 4.8 percent per year, which is statistically insignificant. Estimated growth rates and effects for nursing, aide, and physical therapy visits follow patterns very similar to the overall results. Given the very rapid growth in visits occurring over the period for all agencies, the differences between the two groups in both the demonstration and predemonstration periods are negligible. Models in which we controlled for average patient characteristics yielded results very similar to those obtained without such controls.

TABLE 6
EFFECT ON AVERAGE ANNUAL **GROWTH RATES**
IN AGENCIES' NUMBER OF MEDICARE **VISITS**
(Percentages)

	Demonstration Period		Predemonstration Difference in Annual Growth Rates	Impact ^a	t-statistic
	Average Annual Treatment Group Increase	Average Annual Control Group Increase			
Skilled Nursing	19.0	22.6	4.0	-7.6	-.54
Home Health Aide	26.9	29.0	4.9	-1.0	-.41
Physical Therapy	22.2	24.9	2.2	4.8	-.30
All Visits ^b	21.3	23.6	2.5	4.8	-.33

SOURCE: Bishop et al. (1995b), based on data from 47 agencies' Medicare cost reports for three years before and three years during the demonstration.

NOTE AU estimates were obtained from a fixed-effects model using the logarithm of visits as the dependent variable. Increases reported are estimated average annual percentage increases in visits rendered. See Bishop et al. (1995b). Agencies with less than 100 visits in any year were excluded from the models because of their erratic growth paths.

^aImpacts are equal to the treatment-control differences in growth rates during the demonstration (columns 1 minus column 2), minus the predemonstration difference in growth rates (column 3).

^b"All visits" includes those for speech therapy, occupational therapy, and medical social services as well as the three visit types in the table. Separate estimates for some visit types are not presented because many agencies provide so few such visits that percentage growth rates are erratic. The three visit types not examined separately account for only about 4.3 percent of all visits for treatment group agencies (and 3.3 percent for control agencies) in the predemonstration period.

We also find that treatment group agencies did not increase the number of visits per episode in response to **the** demonstration. Treatment group agencies actually delivered substantially fewer visits per episode than control agencies, but this **difference** was due entirely to a single small agency that averaged over 250 aide visits per episode. When all episodes provided by this agency are dropped, the estimated demonstration effects on skilled nursing, aide, and total visits are small and statistically insignificant (see Table 7). Estimated effects on physical therapy visits are statistically significant; however, they imply that treatment group agencies delivered *fewer* visits per episode, a result which is inconsistent with the incentives under prospective rate setting. On the other hand, the estimated effect on medical social services is positive and significant. Although the estimate is in the expected direction, it is unlikely that the difference is due to the demonstration, given the absence of effects on the number of nursing, aide, or other visit types per episode. Medical social service visits account for less than one percent of agency visits, so increasing the number of such visit would have little bearing on an agency's economies of scale or financial performance. We conclude that neither of these differences is likely to be due to effects of the **demonstration**, but rather are indicative of chance differences in the patients treated by the treatment and control agencies during the demonstration period.

We also estimate that prospective rate setting had no effect on the average length of episodes, but reduced the number of visits per week by about one-third of a visit (9 percent). Again the direction of the effect, a reduction in visit frequency, is inconsistent with the incentives under the demonstration. Furthermore, the finding of an effect on visits per week is inconsistent with the conclusions of no effect on either visits per episode or length of episode. The slightly fewer visits per episode and slightly longer episodes for the treatment group agencies (neither of which is statistically significant) combine to produce slightly fewer visits per week (which is significant). The difference in significance arises because the length and visits per episode measures have considerably larger variances than the visits per week measure.

TABLE 7
EFFECTS ON VISITS PER EPISODE

Measures of Utilization Per Episode	Predicted Mean Without Intervention	Impact	t- statistic
Total Visits Per Episode	48.1	-1.1	-.64
Skilled nursing	24.0	.3	.38
Home health aide	17.2	.6	.57
Physical therapy	5.6	-2.1**	-9.20
Occupational therapy	.57	-.09	-1.26
Speech therapy	.22	.03	.48
Medical social services	.41	.08**	2.63
Duration of Episode (Days)	83	2.5	1.03
Visits Per Week	3.77	-0.35**	-24.79

SOURCE: Bishop et al. (1995b), based on data from demonstration claims.

“t-statistics incorporate adjustments to account for our estimate that standard errors are understated by a factor of 3, because of clustering and weighting.

**Significantly different from zero at the .01 level, two-tailed test,

Again, we conclude that **the** regression estimate of the treatment-control difference is not due to the effects of prospective rate setting, but to other differences between the two groups of agencies in practice patterns or in types of patients treated that are not captured by the extensive set of control **variables**.⁵

Finally, we found no demonstration effect on the length of skilled nursing visits, but large and statistically significant estimates of the effect on the length of aide visits. Shortening the length of visits is perhaps the most direct way to reduce cost per visit, and the case study **finding** that treatment agencies expect nurses to complete more visits per day on average suggests that such effects may have occurred. Our survey data collected three weeks after home health admission, shows that the average nursing visit length reported by patients was very similar for treatment and control groups--about 43 minutes. For aide visits, however, average visit lengths were substantially shorter for the treatment **group--65** minutes compared to 83 minutes for the control group (with visit length truncated at 240 minutes). Regression model estimates of program effects controlling for patient characteristics and for whether the patient had certain diagnoses associated with long visits had virtually no effect on this difference, and the estimate was statistically significant at the .05 level. The difference is in the proportion of visits that last one hour or less--54 percent of the control group, 76 percent for the treatment group. This estimate is inconsistent with the finding of no effects on the average costs of aide visits. Furthermore, treatment and control group agencies responding to questions on visit quotas in the first agency survey had very similar means. Thus, we conclude that the observed difference in visit length for aides is probably not due to the demonstration, although such an effect cannot be ruled out.

Treatment Agencies Prospered Under the Demonstration

Although the evidence suggests that treatment group agencies did not reduce their per visit costs measurably or increase their number of visits relative to what they would have been under cost-based

⁵The regression model for visits per week explains 73 percent of the variance.

reimbursement we do find that they profited under **the demonstration**. On the **surface**, these findings seem contradictory—agencies could prosper only by holding their unit cost increases (compared to costs in their base year) below the rate of inflation used by HCFA in setting payment rates during the demonstration (about five percent per year). We reconcile these findings next, after presenting the estimates. The discussion draws on our report on agency net revenues (Thornton 1995), which contains a more extensive discussion of methods and results.

To assess the demonstration's effect on agencies' financial well-being, we estimated the profits of treatment group agencies during the demonstration and compared them to analogous estimates for the control group agencies. Control group agencies could not earn profits on their Medicare business because they were paid on a cost basis. We can, however, compare their actual costs to the revenues they would have received for the Medicare visits rendered had they been paid under the same formula as the treatment group. The comparison essentially assesses the treatment-control **difference** in agencies' decrease in real cost per **visit** relative to their own base year (because revenue is set at adjusted base-year costs).

Revenues for both groups were calculated by multiplying the number of visits of each type in a given **demonstration** year by the payment rate, usually set at the base-year cost per visit for that agency times the **inflation** factor specified by HCFA. If an agency's base-year cost exceeded the HCFA cost limit, however, **its** payment rates were set at a proportion of the base-year costs, so that payments under the demonstration would not exceed the agency's cost limit. For each demonstration year, payment rates were adjusted retroactively **in** accordance with the demonstration formula if the growth or decline in volume exceeded 10 percent. The profit- and loss-sharing provision (described earlier) was then applied to the estimated net revenue for each agency.

We found that treatment group agencies were substantially more likely than control group agencies to earn "profits" (that is, hold costs below actual or hypothetical revenues) on their Medicare visits under these calculations. Table 8 displays the results obtained when the sample was restricted to the agencies

IMPACTS ON NET REVENUES FOR MEDICARE-FOCUSED AGENCIES

TABLE 8

Dependent Variable	Pooled Sample			Year 1			Year 2			Year 3		
	Predicted Mean Without Intervention	Average Annual Impact ^a	Predicted Mean Without Intervention	Predicted Mean Without Intervention	Average Annual Impact	Predicted Mean Without Intervention	Predicted Mean Without Intervention	Average Annual Impact	Predicted Mean Without Intervention	Average Annual Impact	Predicted Mean Without Intervention	Average Annual Impact
Percentage of Agencies with Positive Net Revenues from Medicare Home Health Care	39.5	37.0** (3.62)	35.6	31.6 ^b (2.01)	\$1.94 (1.13)	42.5	38.6* (2.30)	\$3.06 (1.07)	31.3	58.5** (3.72)	-\$1.07	\$4.47** (3.01)**
Mean Net Revenues per Medicare Home Health Visit ^b	-\$1.08	\$2.96* (2.65)	-\$1.25	\$1.94 (1.13)	-\$1.20	\$3.06 (1.07)	-\$1.20	\$3.06 (1.07)	-\$1.07	\$4.47** (3.01)**	-\$1.07	\$4.47** (3.01)**

SOURCE: Thornton (1995), based on data from the Medicare cost reports provided by the 36 Medicare-focused agencies. This group excludes the 1 demonstration agencies that were (1) hospital based, (2) hospice based, or (3) had less than half their visits covered by Medicare. Because a few agencies did not provide reports for all three years, the sample sizes vary across years. Models were estimated separately for each year and for the pooled sample.

^a These t-statistics are overstated because of the pooling of nonindependent observations across the three years. The estimates are still significant when data for the three years are aggregated rather than treated as independent observations, however.

^b Control agencies could not have positive net revenues from Medicare home health care because they were paid their costs (up to the Section 223 cost limits). For analysts of net Medicare revenues, we computed the hypothetical profits control agencies would have earned given the number of visits they actually provided (of each type), their observed costs, and the prospective rates they would have received if they had been assigned to the treatment group.

*Significantly different from 0 at the .05 level, two-tailed test

**Significantly different from 0 at the .01 level, two-tailed test

that were “Medicare-focused,” defined as providing half or more of their visits to Medicare beneficiaries and not being hospice- or **hospital-based**.⁶ Net revenues on services to Medicare patients were positive for 77 percent of agency years for the treatment group (predicted mean plus impact for the pooled sample, in Table 8). This figure compares with 40 percent for the control group after adjusting for differences between agencies, **with** the estimated impact growing during the demonstration period. When compared with controls, treatment agencies on average earned about \$900,000 more per year in gross Medicare revenues (an increase of about 36 percent not shown), \$197,000 more in net revenues **from** Medicare; and about \$3 more in net revenues per Medicare visit (a profit of \$2, compared with a loss of \$1). Impacts tended to be larger for the last year of the demonstration than for the first or second years.

The **finding** that treatment group agencies profited under the demonstration and controlled cost increases better than control group agencies appears inconsistent with the earlier finding that prospective rate setting did not lower costs. There are several possible reasons for the discrepancy between these two related analyses:

- The revenue analysis was limited to the 36 demonstration agencies that were considered **Medicare-focused**; the cost analysis included **all** agencies.
- The revenue analysis applied the appropriate inflation factor and volume adjuster to the **base-** year costs in computing revenues for each year; the cost analysis used actual costs.
- The revenue analysis scaled down the base-year costs per visit if base-year costs exceeded the **limits**; the cost analysis did not.
- The differences between the base year and demonstration years for some agencies were scaled down by the profit- and loss-sharing provisions in the revenue analysis.

⁶We limited the sample to the 36 Medicare-focused agencies because the hospital-based agencies and those with low proportions of Medicare patients tended to have other objectives that dominated and sometimes distorted the incentive to maximize profits and minimize costs. The two hospice-based agencies were deleted because they served a very different patient mix, with much higher mortality rates. Results for all agencies showed a significantly higher proportion of treatment **than** control agencies earning positive net revenues, but no difference in the average size of net revenue per visit, because several of the non-Medicare-focused agencies had large losses.

- The revenue analysis used as the dependent variable the **difference** between an agency's demonstration year cost per visit and its adjusted base-year cost, for each of the three demonstration years. The cost analysis used the deviation between cost per visit in any given year and average cost **for** that agency during a six-year period (three predemonstration, three demonstration years), and included all six years as observations.
- The revenue analysis implicitly used a weighted average of the six visit types to compute average cost per visit in the demonstration and in the base year, with weights equal to each type's share of the agency's total Medicare visits in the demonstration year. The cost analysis provided separate estimates for each visit type.
- The revenue analysis regressions were estimated separately for each year; the cost analysis combined all years and used a fixed-effects model to account for correlation among the observations for a given agency.

The disparity in conclusions about impacts on agency behavior appears to be due to the greater variance in costs per visit than in net revenues. We eliminated each of the differences, one at a time, to identify which changes were responsible for the **difference** in results. The various adjustments for volume, cost limits, and profit sharing shrank the distribution of differences between demonstration and base-year costs. When these differences were removed, the estimated effect on net revenue becomes statistically **insignificant**, but remains positive (that is, costs grew more slowly for the treatment group), consistent with the negative but insignificant estimates of impacts on cost per visit when the cost analysis is limited to freestanding agencies.

We concluded that Medicare-focused, **free-standing** agencies under prospective rate setting were more **likely** to hold their costs below inflation-adjusted predemonstration costs, but the differences were small, yielding an **insignificant** difference in the change in average costs for the two groups. Treatment agencies, were nearly twice as likely as controls to hold their costs below adjusted base-year levels. The relatively modest decrease in average cost for this fraction of agencies was not **sufficient** to lower average costs for the entire treatment group to any significant degree, however. Random variation in cost per visit masked the limited effect that the demonstration had on cost per visit. The limited sample size yielded imprecise

estimates of the size of this expected effect, but we are confident that it was small, on the order of \$3 per visit or about four percent of average cost.

This finding is consistent with demonstration incentives and agencies' limited opportunities to lower their costs, and with our findings from the case studies. Some demonstration agencies under prospective rate-setting made modest changes to become more efficient, but they were limited in what they could do or were willing to do in a temporary demonstration. Thus, cost impacts were limited and not readily discernible.

Other Medicare Costs Were Unaffected

A key concern about the demonstration was whether changes in home health care in response to the demonstration would lead to changes in the need for other Medicare-covered services. For example, if agencies cut back on visit length or the quality of home health care, patients might have been more likely to experience problems that required a hospital admission or physician visits. The cost of these problems could have offset or exceeded any potential savings from lower per-visit costs for home health care. Alternatively, if agencies increased the number of home health visits per episode, patients might have needed fewer Medicare services of other types.

We found that prospective rate setting had no effects on Medicare service use, either during the episode or during the six months after the episode ended, for any of the three demonstration years. We estimated impacts by comparing the patients of treatment and control group agencies according to their use of and reimbursements for hospital care, skilled nursing facility care, home health care, physician visits, and other Medicare-covered services. The results, a few of which are displayed in Table 9, show that rates of service use were quite high for both groups. Patients' average Medicare cost during the episode ranged from \$2,200 to \$2,400 per month, and nearly 40 percent were admitted to a hospital during the six months following the episode. The lack of consistent treatment-control differences suggests that the program did not affect these costs, however. The anomalous and significant differences observed for Part B services

TABLE 9

IMPACTS ON USE OF OTHER MEDICARECOVERED SERVICES

Outcome	Per Month During Episode, for Admissions in:			During Six Months After Episode, for Admissions in:	
	Year 1	Year 2	Year 3	Year 1	Year 2
Medicare Reimbursement					
Mean	\$2,211	\$2,395	\$2,434	\$5,574	\$6,298
Impact	134	219	423*	5	255
t-Statistic ^b	(1.03)	(1.46)	(2.09)	(.01)	(.53)
Whether Admitted to Hospital					
Mean	21.6%	20.9%	20.9%	39.4%	38.7%
Impact	.7	-.5	-2.2	.4	-.5
t-Statistic ^b	(.41)	(-.30)	(-.94)	(.16)	(-.24)
Number of Physician Visits					
Mean	3.2	3.0	3.0	3.8	2.7
Impact	.02	-.1	.4	.1	
t-Statistic ^b	(.08)	(-.43)	(1.13)	(.23)	(1.2)
Whether Received Durable Medical Equipment					
Mean	39.6%	41.1%	38.9%	13.8%	20.7%
Impact	-3.2	-4.3*	-3.4	-.7	-1.7
t-Statistic ^b	(-1.67)	(-2.25)	(-1.28)	(-.49)	(-1.00)
Sample size					
Treatment Group	24,043	23,484	15,881	21,992	20,790
Control Group	9,565	10,175	5,552	8,628	9,001

SOURCE: Schochet (1995). based on data from National Claims History.

NOTE: The study also examined impacts on Part A and Part B reimbursements separately, as well as use of and reimbursements for various other Medicare-covered services (skilled nursing facility care, home health care, hospice care, emergency and nonemergency outpatient services). See Schochet (1995) for complete results.

*Six months of postdischarge data were not available for a significant fraction of year 3 admissions. Thus, we limited this analysis to admissions in the first two demonstration years.

^b t-statistics incorporate adjustments to standard errors to account for loss of precision because of clustering and weighting (see Schochet 1995).

*Significantly different from zero at the .05 level, two-tailed test.

(not shown) were found to be due to extreme mean values for the patients of two treatment group agencies--one agency's patients had **extraordinarily** high average Part B reimbursements; the other's patients had extremely low use rates for Part B **services**.⁷ The significant difference for total Medicare reimbursements in Year 3 also disappears when these agencies are dropped. Schochet (1995) provides a complete discussion of the methods and results.

We also found that the demonstration had no effects on Medicare **service** use for subgroups of patients that we believed were more susceptible to changes the agencies might have made in response to demonstration incentives. Impacts for patients that had diagnoses associated with high home health costs and for patients that had conditions associated with long home health visits were no different from those for patients without such conditions. **Efforts** to contrast impacts for agencies with **different** characteristics (for example, for profit/nonprofit) yielded erratic and internally inconsistent estimates because of the small number of agencies.

EFFECTS ON PATIENTS

Home health patients could have been affected by agency actions in response to demonstration **incentives** in at least three ways: (1) the quality of the home health care delivered could have decreased or **increased**; (2) access to home health care could have decreased; and (3) patients' need for services not covered by **Medicare** could have increased or decreased.

Quality of Care Was Unchanged

If agencies attempt to reduce per-unit costs by shortening visit lengths, using less-experienced staff, or cutting back on expensive services and supervision, the quality of home health care could suffer. On the other hand, increases in visits per episode in response to the demonstration incentives could increase

We found that treatment agency patients were significantly **less** likely to have incurred some Part B costs, but *to have* a significantly *higher* average amount of reimbursements. Both effects disappear when patients **from** the two anomalous agencies are dropped.

quality. We tested for such effects by comparing patients **from** treatment and control agencies according to the process of care, patient outcomes, and patient satisfaction, using three different data sources: (1) **quality** assurance reviews conducted by the New England Research Institute (**NERI**); (2) Medicare claims data; and (3) surveys of patients. Phillips (1995) provides a complete discussion of the methodology and results.

Analysis of the **NERI** data which provided the only measure of the process of care, revealed several significant treatment-control **differences** but no clear indication that the treatment group had received better or worse **care**.⁸ The significant difference in the proportion of cases with potential or actual adverse effects (see Table 10) suggests that quality of care was reduced for the treatment group. Both the proportion of cases with any problem and the proportion of cases with problems that had the potential to lead to adverse outcomes were significantly greater for the treatment groups in the weighted **logit** models. The estimates were heavily influenced by **colinearity** with agency and area control variables, however, and declined in size and became statistically insignificant when these variables were dropped from the model. The estimates in which agencies are represented in proportion to size were also statistically insignificant.

The **NERI** data also contained measures of patient functioning at discharge, based on information in the patient record. Although statistically significant differences were found for 5 of the 12 activities of daily living (ADL) and instrumental activity of daily living (IADL) tasks examined, the estimated effects were inconsistent in direction. The treatment group was less impaired on some measures of functioning at discharge (ability to eat and to ambulate without assistance) and more impaired on others (ability to do housework and laundry without help). Again, none of the estimates was significant in the size-weighted

⁸The quality assurance reviews were conducted by nurses, using a **formal** protocol to extract data **from** patient records. Random samples of patients were selected for each of five “tracer” conditions commonly seen by home health agencies: (1) congestive heart failure; (2) stroke; (3) hip **fracture** or replacement; (4) urinary incontinence; and (5) **decubitus** ulcers of at least stage 2 seriousness. A random sample of patients who have none of these conditions was also selected for quality assurance reviews. See Phillips (1995) for a more detailed description of the samples and outcome measures.

TABLE 10
IMPACTS ON PROCESS OF CARE PROBLEMS AND FUNCTIONAL ABILITY
(Percentages)

Dependent Variable	Not Controlling for Agency and Area Characteristics		Controlling for Agency and Area Characteristics	
	Predicted Mean without Intervention	Estimated Treatment-Control Difference ^a	Predicted Mean without Intervention	Estimated Treatment-Control Difference ^a
Confirmed Quality Assurance Problem				
Problem that had potential for significant adverse effect or that resulted in adverse effect	3.5	1.4 (1.50)	3.2 [*]	2.2 [*] (1.97)
Any problem	43.1	3.0 (1.34)	41.3	6.5 [*] (2.57)
Screen Failure				
Failure to deliver prescribed nursing or therapy	33.2	2.4 (1.10)	31.1	6.7 ^{**} (2.83)
Functional Ability				
Eating: able to eat without human assistance	82.9	-3.3 [*] (-2.45)	83.0	-3.7 [*] (-2.51)
Transferring: able to transfer without human assistance	79.3	-3.4 [*] (-2.27)	79.5	-4.0 [*] (-2.43)
Ambulating: able to walk/wheel without human assistance	82.5	-3.2 [*] (-2.38)	83.1	-4.4 ^{**} (-2.99)
Housework: able to do light housework without human assistance	10.1	2.8 [*] (2.42)	9.0	5.1 ^{**} (3.85)
Laundry: able to do personal laundry without human assistance	9.9	2.4 [*] (2.12)	9.1	4.1 ^{**} (3.14)

SOURCE Phillips (1995), based on reviews of patient records conducted by the demonstration quality assurance contractor (New England Research Institute).

NOTE The sample included all patients for whom quality assurance reviews were conducted, that is, the five tracer condition samples and the non-tracer sample. The observations were not weighted to be representative of all home health patients with regard to the various tracer conditions. The sample size differs by dependent variable, ranging from 3,507 to 3,774 episodes. About 58 percent of observations were for treatment group members.

^a t-statistics for the coefficients on treatment status reported here incorporate adjustments to account for our estimate that standard errors were understated because of clustering and weighting. The standard errors for quality assurance outcomes were understated by 40 percent; those for functioning were underestimated by about 20 percent.

- Significantly different from zero at the .05 level, two-tailed test.
- Significantly different from zero at the .01 level, two-tailed test.

analysis. Also, as we shall see below, survey measures of functioning exhibited no significant **treatment-control** differences. Thus, we concluded that the significant differences in functioning were not due to program effects, but to **differences** in the patients or to unusual values of outcomes for the small agencies.

The outcomes analysis based on Medicare claims data also indicated that the demonstration had no effect on the quality of care. Quality was measured for these analyses by hospital admissions, skilled nursing facility (SNF) admissions, readmissions to home health, and mortality. Each variable was measured for two periods (within 30 days after home health discharge and within six months after discharge). We examined impacts on admissions for any diagnosis and for only diagnoses related to the diagnosis for the original home health admission. Estimating each equation in weighted and unweighted form for each demonstration year and overall yielded 98 estimates.⁹ Table 11 contains some of the key overall estimates.

Only 3 of the 98 estimates were statistically significant at the .05 level, less than would be expected to occur by chance. All estimates of impacts on mortality and hospital admissions were small and statistically insignificant. Estimated impacts on readmission to home health care within one month after discharge for patients admitted in the third demonstration year and on admission to a **SNF** during the six months after discharge were statistically significant, but in opposite directions (not shown). The lack of robustness of these estimates across demonstration years and related outcome measures--signs sometimes differed and magnitudes varied--led us to conclude that the few significant estimates were statistical aberrations, rather than evidence of program effects.

⁹For outcomes measured during the six-month postdischarge period, impacts were estimated for only the first two demonstration years, due to the large number of third-year admissions for which data for the full six month **followup** period were not available in time for the analysis.

TABLE11
EFFECTS ON POST-DISCHARGE OUTCOMES
(Percentages)

Outcomes	At Discharge or Within 30 Days			Within Six Months After Discharge		
	Mean Without Intervention	Impact	t-statistic	Mean Without Intervention	Impact	t-statistic
Whether Deceased	12.7	1.4	1.47	22.0	2.5	1.91
Whether Admitted to Hospital (any diagnosis)	17.1	1.5	1.25	35.8	-1.0	-.59
Whether Readmitted to Home Health (any diagnosis)	4.8	.7	1.05	21.4	-1.3	-.91

SOURCE: Phillips (1995). based on data **from** National Claims History files. Results are for all patients discharged during the three-year demonstration period. The sample size **was** approximately 78,513 episodes. The t-statistics are adjusted to account for underestimation of standard errors resulting from clustering and weighting.

Finally, the patient **survey** data revealed no statistically significant differences in functioning at either discharge or six months later (regardless of weighting), and patient satisfaction measures suggested that demonstration effects on quality were either positive or zero. None of the functioning measures from the survey (Table 12) exhibited sizable treatment-control differences at either point, and there was no **difference** in use of hospital or physician care for unexpected problems. The satisfaction measures related to instructions on care, overall quality, complaints, and unmet needs showed no treatment-control **difference** when observations were weighted to reflect an agency's share of episodes. When observations were weighted to give each agency equal weight (Table 13), the proportion of patients rating overall quality of care as good or excellent and the proportion saying that the staff spent adequate time on care during home visits were significantly greater for treatment agencies, indicating *better care* for the treatment group.

Given the lack of consistent evidence across years, outcome measures, weighting methods, and data sources, we concluded that the program had no discernible effect on quality in either direction. If effects did exist, they were just as likely to be quality improvements as reductions. The lack of effects on quality was consistent with the **finding** that agencies did little beyond what they would have normally done to control costs

Agencies Did Not Limit Access to Care

The evidence also suggest that treatment group agencies did not deny admission to patients who would require long or expensive visits. They also did not **try** to transfer these patients or to discharge them sooner than under cost reimbursement. We examined these issues by using demonstration claims, patient intake forms, and care plan data to (1) test for whether the patients admitted to treatment agencies differed significantly from control agency patients on characteristics at admission that might be associated with a particularly high (or low) cost per visit, and (2) test for effects on whether demonstration agency patients were admitted to a different home health agency shortly after discharge from the demonstration agency. Klein and Brown (1995) provide a detailed discussion of the methods and findings summarized here.

TABLE 12
IMPACTS ON SURVEY MEASURES OF FUNCTIONING
(Percentages)

Functional Impairment (Usual Performance of Task During Two Weeks Preceding Interview)	At Discharge		six Months After Discharge	
	Predicted Mean Without Intervention	Estimated Treatment-Control Difference ^a	Predicted Mean Without Intervention	Estimated Treatment-Control Difference ^a
Had Human Assistance with Eating	26.6	0.1 (.04)	18.9	1.5 (.61)
Had Human Assistance with Transferring	39.8	-1.4 (-.49)	33.3	-5.0 (-1.65)
Had Human Assistance with Walking/Wheeling	34.3	-3.1 (-1.07)	24.3	-3.5 (-1.24)
Had Human Assistance with Bathing	56.2	-3.6 (-1.37)	47.3	-0.8 (-.27)
Had Human Assistance with Taking Medication	52.6	-0.1 (-.03)	45.8	1.3 (.41)

SOURCE Phillips (1995). based on data from telephone surveys of patients conductal at home health discharge and six months after discharge.

NOTE Sample sizes for these outcomes ranged from 1,119 to 1,473 episodes.

^at-statistics (in parentheses) are based on standard errors adjusted to account for our estimate that they were understated by 20 percent because of clustering and weighting.

TABLE 13
IMPACTS ON SATISFACTION WITH CARE
(Percentages)

	Predicted Mean Without Intervention	Estimated Treatment- Control Difference'
Comfort Level with Instructions on Care		
Very comfortable	68.9	1.6 (.41)
Comfortable or very comfortable	89.2	1.5 (.58)
Overall Quality of Care		
Excellent	69.0	-1.2 (-.35)
Excellent or good	93.9	4.5** (2.65)
Patients' Assessment of Care		
Any complaints about care	9.4	-2.4 (-1.06)
Staff came on time	96.3	-0.8 (-.54)
Staff worked deliberately (did not rush)	91.7	4.6* (2.27)
Unmet Need		
Needed more care than received	21.8	-1.9 (-.68)

SOURCE: Phillips (1995), based on data from telephone surveys of patients conducted at home health discharge.

NOTE: The sample size differed by dependent variable, ranging from 1,119 to 1,473 patients.

*t-statistics (in parentheses) are adjusted to account for our estimate that standard errors were understated by 20 percent because of clustering and weighting.

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

Although we observed statistically significant differences in a few instances, there was no pattern in the estimates to suggest that agencies became more selective about the patients they accepted as a result of switching to prospective rate setting. Treatment and control group patients differed significantly in all three years on only two of the many patient **characteristics** at admission--clinical stability and preadmission location (See Table 14.) Furthermore, the clinical stability **differences** suggested that treatment agencies had **sicker** patients rather than healthier ones. The two groups also **differed** for one or two years in several other characteristics, but the differences appeared to be random rather than indications that treatment agencies altered their patient-mix in response to the demonstration. The significant treatment-control differences observed in all five ADL measures for year 1 admissions were absent for year 2 and year 3. We attributed the year 1 differences to the fact that treatment agencies had previously served caseloads with more acute and fewer chronic illnesses than control agencies (consistent with the finding that treatment agency patients were more likely than control patients to have been in a hospital just prior to **admission**, and less likely to have been in a nursing home or private residence). These differences gradually declined over time for reasons unrelated to the demonstration.

We also found no meaningful differences between treatment and control patients in the proportion **with** characteristics that are associated with long visits (see Table 15), or in the average episode length for such patients. **Again**, the one exception suggests that treatment agencies were **more** likely to admit patients **with high-cost visits**. We also found no significant treatment-control differences in average episode length for any of the eight subgroups of high-utilization patients examined (not shown), or in the percentage of **patients admitted** to a different home health agency within three months after discharge from the demonstration agency. Treatment agency patients were less likely than controls to reside in unsafe neighborhoods, but the difference was due mainly to **predemonstration** differences in the proportion of these patients admitted.

TABLE 14

TREATMENT-CONTROL DIFFERENCES IN PATIENTS' CHARACTERISTICS AT ADMISSION
(Percentages, unless Stated Otherwise)

Dependent Variable	Year 1		Year 2		Year 3	
	Predicted Mean Without Intervention	Estimated Impact	Predicted Mean Without Intervention	Estimated Impact	Predicted Moan Without Intervention	Estimated Impact'
Functional Impairments (Requires Assistance with)						
Eating/Tube Feeding	46.9	-8.1**	36.4	-1.4	32.9	0.3
Transfer	69.4	-7.4**	64.0	-2.6	63.7	-1.9
Toileting/Elimination	60.4	-7.3**	55.7	-2.4	55.0	-1.1
Dressing	76.9	-8.1**	73.8	-3.9	71.9	-1.1
Bathing	84.3	-6.6**	81.6	-3.4	80.6	-2.4
Bowel or Bladder Incontinence	21.6	-2.6	21.3	-3.0	23.1	-7.1**
Ambulation Impairment	79.8	-3.7	78.2	-2.2	76.7	-5.7*
Endurance Impairment	90.3	0.5	88.3	0.4	87.0	0.3
General Health Status						
Pm-AdmissionLocation						
Home or apartment	34.1		37.1	4.9	38.9	-2.5
Nursing home or rehabilitation hospital	13.5	-5.4	13.3	-4.0	13.9	-3.9
Acute care hospital	50.3	4.8	48.7	7.4	45.8	4.1
Other (congregate care home or other supported living facility)	2.1	0.6	0.9	1.5	1.4	2.3
Number of Hospitalization in Past 12 Months	1.5	0.1	1.5	0.0	1.4	0.1
Clinically Stable	37.2	-5.8*	42.1	-7.0**	42.6	-9.0**
Medicare Expenditures in Previous Year (In Dollars)	14,297	-446	17,369	-523	17,858	247
Improvement Expected	45.0	0.7	46.8	-1.0	46.4	-5.1
Sample Size	33,750		33,658		30,039	

SOURCE: Klein and Brown (1995), based on data from the patient intake form and National Claims History files.

*In performing hypothesis tests, t-statistics (not shown) were adjusted to account for our estimate that standard errors for this sample were underestimated by a factor of 3.46 as a result of clustering and weighting.

*Estimates of program impacts on pre-admission location were obtained from a multinomial logit model.

*Significantly different from zero at the .05 level, two-tailed.

**Significantly different from zero at the .01 level, two-tailed.

TABLE 1 s
IMPACTS ON THE ADMISSION OF PATIENTS LIKELY TO REQUIRE LONG VISITS
(Percentages)

Year 1			Year 2			Year 3		
Predicted Mean Without Intervention			Predicted Mean Without Intervention			Predicted Mean Without Intervention		
Estimated Impact ^a			Estimated Impact ^a			Estimated Impact ^a		
Daily Intravenous (IV) Injections/Infusion	1.8	0.4 (.59)	1.6	0.7 (1.02)	2.7	-0.4 (-.34)		
Primary Diagnosis: Serious Cancer	6.3	3.6 (2.36)	9.3	-0.7 (-.48)	8.7	-0.5 (-.33)		
Primary Diagnosis: Complicated Wounds	6.1	-1.3 (-1.09)	6.4	-1.7 (-1.38)	5.2	0.5 (.37)		
Expected Outcome: Terminal Illness	3.3	0.9 (.72)	3.9	-0.3 (-.23)	3.5	-0.3 (-.21)		
Deceased Within 90 Days After Admission	11.2	1.2 (.75)	10.9	-0.6 (-.37)	9.6	0.8 (.44)		
Impaired in Transfer and Serious Decubiti	1.4	0.0 (.00)	1.5	-0.3 (-.45)	1.2	0.0 (.00)		
Administer Tracheostomy Care	0.6	-0.4 (-.67)	0.1	0.2 (.53)	0.9	-0.9 (-.85)		
Gastrostomy Care and Enteral Feeding	0.8	-0.2 (-.38)	1.1	-0.6 (-1.09)	0.9	-0.5 (-.81)		
Sample Size	33,750		33,658		30,039			

SOURCE: Klein and Brown (1995), based on data from patient intake and plan of treatment forms.

^aStatistics (in parentheses) are adjusted to account for our estimate that standard errors were understated by a factor of 3 because of clustering and weighting.

*Significantly different from zero at the .05 level.

Non-Medicare Costs Were Unaffected

Finally, we **found** that any changes agencies made in response to prospective rate setting had no effect on patients' use of services not covered by Medicare, or on the burden borne by patients' informal caregivers either during or after the home health episode (see Table 16). Treatment group patients used no more nursing home care or other formal services not covered by Medicare than control group patients; they also did not receive more informal care from family and friends. Models estimated on survey data collected at home health discharge and six months later showed small and statistically insignificant differences between treatment and control agency patients in receipt of aide/homemaker services, **home-**delivered meals, transportation to a medical care provider, and residence in an assisted-living facility. Similarly, the **statistical** evidence showed that the proportion of patients receiving informal care **from** their families did not increase, whether from visiting or live-in caregivers. About 15 percent of both groups used nursing home care **after** discharge, so this measure also seemed to be unaffected. We did find a large and significant estimate of the effect on nursing home use for a subset of patients that had characteristics associated with high costs per visit (not shown). The estimate implied, however, that nursing home use was *reduced* for these patients as a result of the demonstration, which is the opposite of what would be expected for this subgroup and inconsistent with our estimate that home health visits per episode month for this subgroup were unaffected by the demonstration (Schochet 1995). Thus, we discounted these estimates and ascribed them to chance and the small sample size for this subgroup. See Brown and Klein (1995) for a full presentation of the estimates and methodology.

IMPLICATIONS FOR MEDICARE HOME HEALTH CARE

TABLE 16

IMPACTS ON USE OF HOME- AND COMMUNITY-BASED SERVICES
(Percentages, except as noted)

Home-and Community-Based Services	During Home Health Episode			After Home Health Episode		
	Predicted Value Without Intervention	Estimated Impact	t-Statistic *	Predicted Value Without Intervention	Estimated Impact	t-Statistic'
Nursing Home Use						
Any nursing home admission				14.9	0.9	.38
Nursing home days, for users (number)				93.5	2.7	.13
Formal Care Received						
Any formal care (non-Medicare)	41.0	-2.3	-.83	38.5	3.2	.97
Aide/homemaker services (non-Medicare)	22.6	-0.4	-.17	24.6	-0.0	-.19
Home-delivered meals	15.7	-2.6	-1.30	12.4	0.3	.12
Transportation to medical services	7.9	-1.3	-.73	8.6	0.9	.42
Resident in personal care home	6.8	1.7	1.01	4.1	2.1	1.30
Informal Care Received						
Unpaid care from family or friends	79.6	-4.4	-1.70	66.7	-5.0	-1.45
Live-in caregiver	55.4	-1.6	-.50	49.3	4.1	-1.19
Visiting caregivers	44.1	-0.1	-.03	40.4	-0.0	-.02
Sample Size	1,449 - 1,486			1,219 - 1,262		

SOURCE B- and Klan (1995), based on telephone surveys of patients conducted at home health discharge and six months after discharge.

* Statistics are adjusted to account for our estimate that standard errors were understated by 20 percent because of clustering and weighting.

- Significantly different from zero at the .05 level, two-tailed test.
- * Significantly different from zero at the .01 level, two-tailed test.

increasing computerization and might have **expected** their **staff to** complete slightly more visits per day, on average. The behavioral changes reported by the plans were minimal, however. **Among** agencies that were not facility-based and were focused mainly on Medicare, those subject to prospective rate setting were more successful than control agencies in holding down increases in their per-visit costs relative to the base year. This success enabled about three-fourths to **earn** a profit under the demonstration. The reductions in inflation-adjusted costs were small, however, too small to show up as a significant **treatment-control** difference in the comparison of demonstration with **predemonstration** costs.

This lack of effect is not surprising, given our case studies finding that agencies felt they had little opportunity to reduce their costs. Stiff competition among agencies for staff and for referrals from hospitals and providers limited agencies' ability to reduce **clinical** costs. Putting pressure on staff to complete more visits per day or holding down pay increases could result in loss of staff to competitors. Reducing visit length could also lead **staff** to seek employment elsewhere and could reduce referrals if patients report to physicians that they are dissatisfied with the home health care they received. Furthermore, one-third of the agencies were already **having** difficulty holding costs below the limit before the demonstration, and those with low costs may have had little opportunity to cut costs further,

On the other hand, prospective rate setting did not lead to adverse effects on either total Medicare costs, quality of or access to care, or costs to patients and their families. Total Medicare costs could increase **if** agencies increase the number of visits they provide per patient in response to the opportunity to earn profits. Our **findings** were not consistent with such behavior, however. We found no evidence of decreases in quality, a result consistent with the qualitative and quantitative evidence suggesting that any additional actions to reduce costs were minimal. The absence of effects on quality is also consistent with our finding that treatment agency patients did not require more care **from** informal caregivers or community service organizations than the control agency patients. Home health patients who required expensive visits received no less care under prospective rate setting than under cost reimbursement. The results also

showed, however, that beneficiaries did not experience any of the potentially *beneficial effects* on quality and non-Medicare costs that might have arisen had the number of visits per episode increased.

Low Participation and Demonstration Design Weaken Conclusions

These findings, while internally consistent and in accord with the agencies' reports about their responses to prospective rate setting, **suffer** from one major flaw-the study's low statistical power to detect small to moderate impacts. Although we analyzed data on thousands of patients, they were drawn **from only** the 47 agencies that participated in the demonstration. The small number of agencies, together with the large variation across agencies and over time for individual agencies (especially the small ones), limited the precision of the overall estimates and essentially eliminated the potential to assess whether prospective rate setting worked better for certain types of agencies. The sample size was further reduced for some analyses when facility-based agencies were excluded because they often had broader objectives that conflicted with the expected goal under the demonstration of reducing per-visit cost.

The voluntary nature of the demonstration may have also biased the findings. Agencies that declined to participate typically cited as reasons their fear of losing money under the demonstration, the limited **opportunities** they perceived **for** reducing cost per visit (for example, because of competition for staff and referrals), **concerns** about the costs of participation, and an aversion to switching fiscal intermediaries for three years. If the agencies that entered were those most able to prosper under the demonstration rules, the **estimates** could overstate the effects of a mandatory program of prospective rate setting. On the other hand, **if** most agencies that entered expected their costs to fall for other reasons (such as computerization of records or changes in **staffing** patterns), the effects of national prospective rate setting could be underestimated here. Under this scenario, control agencies' costs would grow more slowly than the typical agency. Given that we find no major effects on costs, it is clear that the change in payment structure was not an effective way to reduce costs for the group of participating agencies. If nonparticipating agencies

were not as motivated to control their costs, prospective rate setting might provide the incentive they need to adopt more effective practices.

A third difficulty with the study is that agencies' behavior under the limited duration of the demonstration might have differed **from** what behavior would be under permanent prospective rate setting. On the one hand, agencies might have been reluctant to make major cost-reducing changes (such as eliminating **staff**) that they would not want to sustain when returning to cost-based reimbursement after the three-year demonstration. Conversely, agencies might have postponed until after the demonstration changes that they needed to make in the long run, in order to earn profits during the limited demonstration period. For example, agencies that grew rapidly during this period (as many did) might have delayed adding additional administrators and supervisors until returning to cost-based reimbursement. It appears that agencies spent most of their effort on managing their rapid **growth** rather than controlling cost.

Basing an agency's prospective rates on its own past experience rather than on national or regional averages also might have distorted inferences about what would occur under national prospective rate setting. If agencies were forced to lower costs relative to some minimally efficient standard (as would be likely under a national system), some might have become more aggressive than they were under the demonstration. Conversely, those with already low costs could have profited without changing their behavior at all. Paying all agencies some percentage of the current national average would generate national savings (provided that this process does not prompt increases in volume). The demonstration provided no guidance, however, about how many agencies would be able to survive under such a system, the types of agencies that would have the most trouble adapting, or the problems that could arise with quality and access if agencies' costs in excess of the mean were not covered.

Finally, there is a basic flaw in any payment approach or cost limit based on visits: Visits can vary widely (and arbitrarily) in content and cost. For example, skilled nursing visits for intravenous therapy typically take much longer and may require more experienced staff than visits to change bandages or draw

W blood Similarly, home health aide visits can be very short (for example, to turn a bed-bound patient) or can last several hours (to help patients with bathing, toileting, and eating). **Furthermore**, whether agencies provide a given type of care in multiple short visits or a single longer visit is **often** at their discretion. We saw no evidence that agencies exploited this opportunity to increase revenues (perhaps because of the volume adjuster), but the possibility clearly exists. Variation in visit length and content could explain some of the current variation across agencies in per-visit costs.

Policy Implications

The recent explosion in the number of visits per episode is the primary source of the large increases in Medicare home health costs, and setting a fixed reimbursement rate prospectively for home health visits would not reverse this trend, even if it did lower unit costs. The demonstration results suggest that the effects of prospective rate setting on costs per visit were limited at best, because agencies had to compete for staff and referrals, and this competition--plus regulations and other factors--constrained their ability to make major reductions in costs. Furthermore, agencies already had the incentive to hold their costs below the cost limits

Nonetheless, there are important lessons to be learned **from** this demonstration. Agencies can make **some** changes to slow the rate of increase in their per-visit costs. These actions will be important under any system of payment, because even small decreases in average costs could save millions of dollars for Medicare. The wide range in costs per visit across agencies and the existence of sizable economies of scale suggest that lower costs per visit are attainable for many agencies. The demonstration suggests that when paid a prospectively set rate, most Medicare-focused agencies found a way to hold average cost increases at least slightly below the inflation rate used by HCFA, and they were nearly **twice** as likely to do so as agencies under **cost** reimbursement.

Moreover, policies directed at reducing the number of home health visits may turn out to be less desirable than ones directed at reducing per-visit costs, because of the greater potential for adverse effects

on quality of care and access. **Shaughnessy** et al. (1994) found that HMO patients had much shorter home health episodes but were more impaired at discharge than home health patients with traditional **fee-for-service Medicare coverage**. Although setting per-visit rates prospectively would not be expected to reduce costs substantially, it might be a safe interim strategy for encouraging agencies to moderate their cost increases and **for** getting high-cost agencies to bring their **costs** more in line with those of other agencies. This approach would allow time to develop and test quality assurance procedures for ensuring that agencies paid a per- episode or per-beneficiary capitation do not underserve their patients, and to develop an adequate case mix adjuster so that agencies are paid fairly for the types of patients they serve. If prospective rate-setting were adopted, however, it may be necessary to have some provision, such as the volume adjustments to payment rates used during the demonstration, to encourage agencies to limit increases in visits per episode. It would **also** be important to develop a usable definition of what constitutes a home health visit.

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APPENDIX A

**REPRESENTATIVENESS OF
PARTICIPATING AGENCIES
ON PATIENT AND AREA
CHARACTERISTICS**

TABLE A. 1

**PATIENT CHARACTERISTICS OF PARTICIPATING AND ELIGIBLE
NONPARTICIPATING AGENCIES, 1990**

characteristic	Participating Agencies	Eligible Nonparticipating Agencies	Difference	P- Value
Number of Agencies	47	1 9 9 2		
Demographic Characteristics'				
Percent Over 80	40.2	37.8	2.4	.08
Percent Female	65.6	63.8	1.8	.13
Percent Black	6.9	10.2	-3.3	.15
Percent with Disability Entitlement	5.2	6.0	-0.8	.21
Medicare Service Use during Year Prior to Admission'				
Home Health Visits-Average	9.2	11.4	-2.2	.17
Home Health Reimbursements	\$499	\$573	-\$74	.30
Part A Reimbursements	\$8,774	\$8,566	\$209	.61
Characteristics at Admission'				
Percentage of Patients Discharged from Hospital within 14 Days Before Home Health Admission	54.5	54.5	0.0	.99
Proportion of an Agency's Patients with Diagnoses Associated with High Home Health Use	4.9	5.0	-0.1	.84
Percent Admitted from SNF	8.7	6.7	2.0	.09

Source: Medicare claims files. Eligible nonparticipating agencies include (1) all agencies that operated in the five demonstration states and met the eligibility criteria of having three years experience and not being government owned, plus (2) a random sample of agencies that met the criteria but operated in other states.

*Average characteristics are calculated by identifying the set of beneficiaries admitted to HHAs during 1990, identifying their characteristics at admission and constructing for each patient the Medicare-use variables for the year prior to admission. The average of each variable was computed for each agency. Averages of these mean values across the set of participating and nonparticipating agencies were then compared.

TABLE A2

**AREA CHARACTERISTICS OF PARTICIPATING AND ELIGIBLE
NONPARTICIPATING AGENCIES, 1990**

Characteristic	Participating Agencies	Eligible Nonparticipating Agencies	Difference	p- Value
Number of Agencies	47	1992		--
Rural	12.8	34.2	-21.5	.00**
Physicians per 1,000 Population	21.3	18.3	3.0	.11
MSA Size (in thousands)				
< 100	14.9	36.1	-21.3	.00**
100-500	27.7	22.9	4.8	.44
500-1,00	4.3	11.8	-7.5	.11
≥ 1,000	53.2	29.2	24.0	.80**
HCFA Hospital Wage Index	1.00	.90	.10	.00**
Average Hospital Occupancy Rates	59.7	59.9	-.1	.95
Average Number of Nursing-Home Beds/1,000 Elderly	58.5	61.4	-2.9	.46

SOURCES: Provider of Services file, Area Resource file, Federal Register (for wage index).

NOTE: Differences between participating and nonparticipating agencies are due entirely to the difference in the proportion of rural agencies. None of the comparisons are statistically significant when eligible nonparticipants are limited to agencies in the five demonstration states and rural agencies are limited to those in Texas and Illinois, the only two states with rural agencies participating in the demonstration.